mental analysis of reasoning: A review of Gilovich's *How We Know What Isn't So. Journal of the Experimental Analysis of Behavior, 64,* 111–116.

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REMINISCENCES OF A REFORMED PIGEON PUSHER

It is impossible for me to separate my work as a research assistant in the Harvard Pigeon Lab from my experiences with Fred Skinner. Before I joined the lab, I was told by one of his former assistants at Indiana University what I should expect. He warned me that I would be given responsibility to find ways to achieve a particular behavioral objective by whatever mechanical, electric, or other means I could find, and that it would be up to me to find ways to succeed at it. One had to grasp whatever Fred had in mind by way of a research objective and then find a way to achieve it. The emphasis was on initiative and originality. No one could complain that his assistants worked within a straitjacket. The atmosphere of freedom of inquiry in which we all worked in that environment was the salient feature of those years for me. As a 2nd-year graduate student, I completed two studies using rats and pigeons before settling on my thesis problem. Both of these were subsequently published, and when I apologized that my acknowledgment for support from the lab had been lost somewhere in the shuffle, Fred replied reassuringly, ''We don't exact tribute here.'' Independence of effort was not only encouraged; it was expected.

The program of the Pigeon Lab gave experimental psychology its flagship research in the field of learning. Not apparent at the time to those of us preoccupied with the effects of schedules of reinforcement, species-specific behavior, differential reinforcement of low and high rates, and rigging ping-pong demonstrations was the subtle influence of Skinner's concept of the operant, which implicitly defined what a true science of human behavior must eventually become. Although the research program shifted focus several times from studies of the effects of schedules of reinforcement to implications of aversive control, the fine structure of visual discrimination, drug effects, and the like, the prevailing theme was that of inductive pragmatism. Regardless of the occasionally impressive swirls of theoretical obfuscation that typified those times, we all knew that what we were doing ''worked.'' That, plus Skinner's oft-repeated observation, ''The subject is always right,'' kept us close to the language of the data. Fred Skinner was impatient with abstract philosophical arguments mainly because he understood their implications so clearly. I recall how the objection by some of our colleagues that Gödel's proof challenged the validity of empirical research was dismissed with a snort to the effect that, like it or not, the experimental approach worked, and so it did. On another occasion, Skinner expressed irritation that another colleague had once made the point that despite his protestations, he did indeed have a theory. His reply was that if thinking the sun is going to rise tomorrow because it always has is a theory, then he guessed he did have one. In any case, he was never opposed to theory as such, only to bad theories.

In the years following, I have often found myself describing the work of the lab to my own students as a place where anything could find its place into the body of science, no matter how unexpected. There was no overriding preconception that ruled where research should or should not go. All that new facts needed for admission to scientific respectability was that they meet minimal operational requirements. New concepts had to be publicly replicable to be verified and accepted.

Programmatic research of the kind pursued in the Pigeon Lab is now rare or impossible for many reasons. One is the ''flight from the lab'' that Skinner himself decried. Psychology is a field entranced with pop culture and quick fixes. Cognitive science, when it is not resurrected structuralism or committed to proving that computers think just like

we do, is what Skinner once called methodological behaviorism. Its practitioners still are in the business of inventing intervening variables: Only the names of the intervening variables have changed. ''Theories'' that sprout like mushrooms, rather than basic research, are currently the popular roads to fame and tenure. The operational details of research on the behavior of nonhuman organisms offer little to practitioners who are practically concerned with knowing which of their customers is most likely to jump off a bridge this week, and in the present climate of ''relevance,'' funds to pursue basic lab research are hard to come by. The Pigeon Lab was initially supported by money given for answers to the question of how most effectively to steer a bomb or nuclear missile to a remote target. Those who funded the Pigeon Lab were almost certainly innocent of the very real advances in the study of behavior that their grants subsidized.

Of course all of this activity went forward with our implicit assumption that something like Newtonian determinism was the appropriate paradigm for all scientific inquiry. Nonetheless, day-to-day observations continually revealed that behavior, whether that of rats, pigeons, or humans, violated one of the prime implications of ontological determinism; namely, that the behavior of an organism reverts to the steady state that prevailed before an intervention. If there is a fundamental truth about operant behavior, it is that it is a constantly evolving process characterized by an unending series of divergences. No organism can ever be the same as it was before its behavior was selectively reinforced. Skinner certainly did not propose the concept of a class of ''emitted'' behavior to anticipate quantum mechanics. The concept was simply the honest concession of the fact that we do not know, nor *can* we know, the specific eliciting stimulus that is responsible for the occasion of a particular operant response. The research conducted in the Pigeon Lab would be valid even if the new physical paradigm had been recognized and broadly accepted as the appropriate paradigm for behavioral research. In this respect, its work stands alone as a model for the new century and beyond.

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Gene M. Heyman (1970–1976)

THE HARVARD PIGEON LAB, 1970–1998: GRADUATE STUDENTS AND MATCHING LAW RESEARCH

In 1970, the year I began graduate school, the Pigeon Lab occupied about half of the seventh floor of William James Hall. The animal colony took up the center room, and the shops and ''running'' rooms, filled with experimental chambers and relay racks, formed the periphery. The heart of the lab was the collection of relay racks with their electromechanical counters, steppers, clocks, and timers. Linked by relays and wires, these devices counted behavior and doled out rewards. It looked like science but also a little like a Rube Goldberg cartoon. In the spirit of the latter image was the laboratory legend that Skinner once tried to dampen the action on his feeders by coating them with Karo® syrup.

Graduate Student Education and Interest in the Matching Law

In the Pigeon Lab graduate students had free rein. We had easy access to equipment and animals and pursued our interests with little overt direction from the faculty. For a while the lab technicians, paid by Herrnstein's grants, even ran our experiments. We weren't apprentices but new researchers. Under this laissez faire educational system, research projects were varied and sometimes idiosyncratic. In the 1970s they included projects on autoshaping, taste aversion, delay of reward, concept formation, visual discrimination, and foraging. The matching law, however, in its encompassing single- and con-